Some Aspects of Determining New Motor Vehicle Engine Emission Levels

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Automobile manufacturers are certifying new motor vehicles and new motor vehicle engines in accordance with regulations established by the Environmental Protection Agency (EPA) for the control of air pollution. These EPA regulations are contained in Title 40 of the Code of Federal Regulations (CFR)—Protection of Environment, Part 85.

The emission standards set limits on exhaust emissions, evaporative emissions and crankcase emissions. For example, emission certification levels for 1973 and 1974 light duty vehicles for hydrocarbons, carbon monoxide, and oxides of nitrogen as measured by the constant volume sample-cold test procedure (CVS-C) were 3.4, 39.0, and 3.0 g/mi, respectively. In fuel evaporative emission tests, the hydrocarbons were not to exceed 2 grams and no crankcase emissions were permitted to be discharged into the ambient atmosphere from any new motor vehicle.

The certification procedure is described in 40 CFR, Part 85. It addresses such matters as application for certification, approval of procedure and equipment, required data, selection of test vehicles, vehicle and engine preparation, gasoline specifications, chassis dynamometer driving schedule, emissions sample procedures and equipment, information to be recorded, calculations of emissions, compliance with emissions standards, and testing by the EPA Administrator.

Of the guidelines provided in the certification procedure, this paper will focus on certification to exhaust emission standards.

Certification to Exhaust Emission Standards

Certification test vehicles designated in the regulations as durability data vehicles are driven, with all emission control systems installed and operating, for 50,000 miles or such lesser distance as the EPA Administrator may agree to as meeting the objectives of the test procedure. Emission tests are to be conducted on these vehicles after 4,000 miles of driving and at accumulated mileages that are multiples of 4,000 miles. (The mileage intervals increased to 5,000 miles for 1975 light duty vehicles.) Additionally, test vehicles designated as emission data vehicles are required to be driven 4,000 miles with all emission control systems installed and operating. Emission tests are to be conducted on emission data vehicles at zero miles and 4,000 miles. Fifty thousand-mile emission levels for each emission data vehicle are computed by multiplying the 4,000-mile exhaust emission test results by a factor. This multiplier is called the deterioration factor (DF), and is computed from the emissions data produced by the durability data vehicles. It is expressed as:

$$DF = \frac{\text{exhaust emissions interpolated to 50,000 miles}}{\text{exhaust emissions interpolated to 4,000 miles}}$$

Values for the numerator and denominator of this ratio are required to be taken from a straight line, like the one shown in Fig. 1, where all applicable HC measurements made on a durability data vehicle are plotted as a function of the mileage on the system. It should

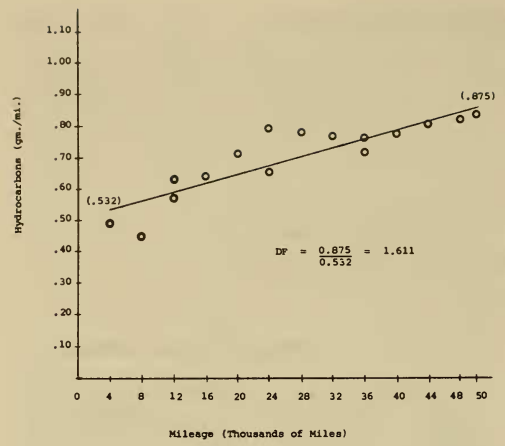

Fig. 1.—Graph for determining deterioration factor (Durability Test Vehicle No. 1012—for hydrocarbons).

be noted that HC measurements were made at 4,000 miles, mileages that are multiples of 4,000, and at mileages where scheduled major maintenance (e.g., tune-up point) of the durability vehicle took place. In the case of major maintenance both before and after maintenance tests are included.

The straight line fitted to the emission data is to be a least squares best fit straight line. The interpolated exhaust emissions that are required for determining the deterioration factor are defined as the 4,000-mile and 50,000-mile intercepts on this line. In Fig. 1, their values are 0.532 g/mi of HC and 0.875 g/mi of HC, respectively. Their ratio, 0.875/0.532, provides a DF value of 1.611 obtained from HC measurements on durability test vehicle No. 1012. As noted earlier, to determine compliance of an emission data vehicle (4,000-mile vehicle) of the same emission system combination, the 50,000-mile HC emission level is estimated by multiplying its 4,000-mile HC emission level by the DF, 1.611. This extrapolated emission value then must be below the applicable acceptance level.

In the event that a durability data vehicle is not tested to 50,000 miles (with the approval of the EPA Administrator), the data for mileages greater than that actually run are to be determined by extending the line of best fit established for the test data at lesser mileages.

Further, it should be noted that if a deterioration factor as determined by the aforementioned method is less than 1, then according to a rule stated in 40 CFR, Part 85, that deterioration factor shall be assumed to be one.

Separate emission deterioration factors are to be determined from the emission results of the durability data vehicles for each emission system combination. Also, an individual deterioration factor is to be established for exhaust hydrocarbons, exhaust carbon monoxide, and exhaust oxides of nitrogen.

When the procedures discussed above are followed. the practice gives rise to some interesting questions. These questions are examined in some detail in the next section of this paper.

Discussion of the Procedure for Calculating and Applying the DF

As shown in the preceding section, a deterioration factor for an emission system combination is defined as the ratio of ordinate values of two special points on a line that is a least squares linear fit of emission data collected over 50,000 miles of emission testing on a durability data vehicle.

The deterioration factor developed by 50,000-mile testing of a representative vehicle is then used as a predictor of emission durability characteristics of similar vehicles which are tested only through 4,000 miles. The 4,000-mile levels are projected to 50,000 miles by application of the appropriate deterioration factor.

The purpose of the deterioration factor is to provide a means for predicting emission compliance at 50,000 miles without actually testing all certification vehicles, as selected by the EPA, over the entire 50,000-mile durability test schedule. Thus, cars are tested at 4,000 miles and emission compliance is determined by projecting these 4,000-mile emission levels to 50,000 miles by means of the vehicle emissions system "predictor" (i.e., the applicable deterioration factor).

A curious aspect of the procedure for

determining the deterioration factor from a least squares best fit line is that the line is not used as a regression line in the usual sense. Ordinarily, a regression line used for making predictions is based on certain underlying assumptions about past performance. In this instance it is assumed that the deterioration factor represents deterioration of the emissions system between 4,000 miles and 50,000 miles of operation. However, the deterioration in emission levels that occurs between 4,000 miles and 50,000 miles can be viewed as the difference between the 4,000-mile and the 50,000-mile intercepts on the least squares best fit line (as is the case for evaporative emissions and heavy duty truck exhaust emissions). Such depreciation is not represented by a deterioration factor expressed as a ratio.

As mentioned above, the current method of determining the deterioration factor for exhaust emissions from light duty vehicles is based on a least squares best fit line. Once this line is established, only the ratio of the 50,000-mile to the 4,000-mile intercepts is used for predictive purposes. This approach tends to ignore the significance of the slope of the least squares best fit line. This point can be best exemplified by examining Figs. 2-A and 2-B.

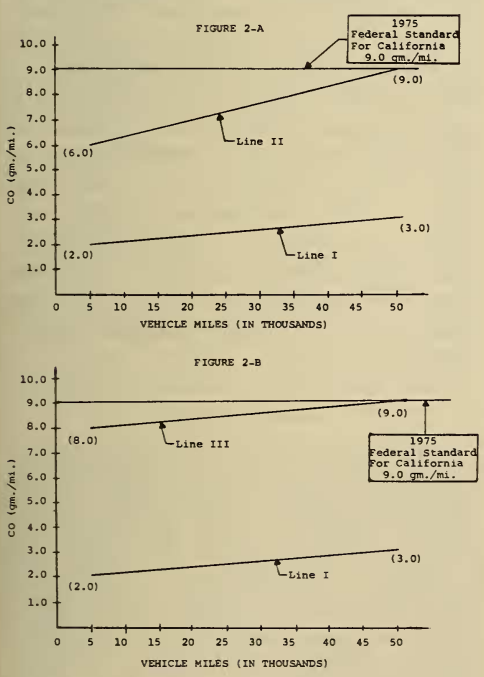

Fig. 2.—Methods of determining the 4,000-mile emission "bogey" level for exhaust emissions from light-duty vehicles.

Let us assume that a 1975 model 50 state certification vehicle was run to 50,000 miles and exhibits emission performance for carbon monoxide as shown by Line I in Fig. 2-A. The 4,000-mile and 50,000-mile intercepts are 2.0 g/mi and 3.0 g/mi respectively, with a resultant DF (ratio) of 1.50. Applying this DF to the emission acceptance level, a maximum allowable 4,000-mile emission level or "bogey" can be generated, which all 4,000-mile emission data vehicles must be less than or equal to. In this example, the CO "bogey" is 6.0 g/mi.

$$\text{4,000-mile bogey} = \frac{\text{50,000 mile standard}}{\text{DF}}$$

$$\text{4,000-mile bogey} = \frac{9.0}{1.5}\ \text{g/mi} = 6.0\ \text{g/mi}$$

Reconstructing an emission performance line based on the 9.0 g/mi acceptance level and 6.0 g/mi "bogey" yields Line II in Fig. 2-A. From a practical engineering standpoint a comparison of Lines I and II of Fig. 2-A indicate that two different deterioration rates exist. However, as defined in 40 CFR, Part 85, the deteriorations have the same representation; i.e., the deterioration factors (when expressed as a ratio) are identical.

Line III on Fig. 2-B can be regarded as more representative of actual emission depreciation on the durability vehicle because it has the identical emission deterioration rate (slope) as the 50,000-mile vehicle. Thus, in this context, a "bogey" of 8.0 g/mi is more appropriate. Interestingly enough, Line I and Line III, with identical deterioration rates, would obviously have significantly different DF's when calculated by the ratio method (1.50 vs 1.125, respectively).

An incongruous feature of the application of the deterioration factor is that the deterioration factor, from the 50,000-mile durability vehicle, can only be used to determine emission system compliance if both the 4,000-mile and 50,000-mile intercepts are below the acceptance levels. Based on earlier discussions of the purpose of the DF (i.e., to project 4,000-mile emission levels to 50,000 miles) and the fact that emission system compliance to standards is predicated on the 4,000-mile emission data vehicle's projected 50,000-mile emission levels being below the standard, it would appear that such a constraint on the DF is rather severe. For, as previously stated, the DF is nothing more than a predictor of emission system depreciation and, as such, should be a valid indicator regardless of the actual emission levels of the durability vehicle selected to represent the emission system.

How good, then, is the DF expressed as a ratio and the 50,000-mile emission projection resulting from its use? Would not the difference between the 50,000-mile intercept and the 4,000-mile intercept of the straight line be a better measure of total vehicle emissions depreciation?

The DF, which is a variable in a statistical sense, is used as shown earlier for setting manufacturer's development objectives. Knowing more about the statistical properties of the DF is essential to finding answers to some of the problems raised in the next section of this paper.

Factors Associated with Setting In-House Emission Development Objectives

Given a 50,000-mile emission level that a certification vehicle should not exceed (acceptance level), a manufacturer may desire to set an in-house development objective for a 4,000-mile emission data vehicle so that he can be reasonably assured that any individual certification vehicle within the emission system will qualify.

To address this problem, a statistician needs to know how measured and calculated emission values tend to be distributed; that is to say, how they tend to vary. He needs to understand, too, to what extent calculated DF's can be expected to vary. Unless the nature of these distributions is known, it is difficult to establish the probabilities implied in the preceding paragraph, and it becomes necessary to consider other statistical approaches to setting emission developmental objectives.

For resolving some of the earlier-mentioned problems, Monte Carlo simulation is a useful approach. This is a methodology that usually requires the assistance of a computer for constructing and sampling distributions from which estimates of the desired probabilities can be extracted. This approach is being used in some quarters.

Summary

Certification criteria make use of a factor that is defined to reflect deterioration of the emissions control system up through 50,000 miles of usage. This paper explains how the deterioration factor is used for projecting a 50,000-mile emission level from an observed 4,000-mile emission level. Recognition was given to the fact that the statistical properties of most of the observed and calculated variables need to be better understood. No attempt was made to provide answers to the statistical questions that were raised in the discussion. Useful approaches to some of the statistical problems were provided in hope that more attention would be given to a statistical base for evaluating emission-related systems.

Acknowledgments

This paper could not have been written without the assistance of Mr. D. N. Hwang, who first brought some of the statistical problems to my attention, and to others in Ford Motor Company who have given these problems considerable attention and who were generous in sharing their views on the problems and their possible resolution. The help of these gentlemen and others from Ford Motor Company is appreciated.

Air Pollutants—Safe Concentrations? Panel Discussion

Chairman: **Dr. John D. Hromi,** *Ford Motor Co.*

Panelists: **Dr. William H. Kirchoff,** *National Bureau of Standards*
Dr. Vaun Newill, *Environmental Protection Agency*
Dr. Nozer D. Singpurwalla, *George Washington University*

DR. HROMI—The theme of this conference is "Statistics and the Environment." We have heard this morning about some problems that suggest a statistical approach to a solution. We hope that, interspersed among the questions today, will be those that pertain to the use of statistics. I don't think that it is intended today that statistics will be presented as a panacea for all problems that confront us today. After Dr. Finklea's presentation this morning we all appreciate the complexity of the control of air pollution. I think, however, it is necessary to establish a perspective that puts statistics in a role of helping to solve some of the related problems. I now ask for questions.

DAVID SALSBURG (Kaiser)—I have a few comments or questions, first of all for Dr. Newill. Has anyone considered or organized planned experiments on a national scale similar to the way advertising news is evaluated? Something like this: You would pair off relatively small communities and take some sort of stratified sample of cars that represented something better than 50%, apply to each community a different method of emission control, then for six months take measurements on ambient air values on short-term health characteristics just to give some kind of well planned experimental design. Secondly, Dr. Hromi. I was impressed by the point you made. I took a quick look at the variance of this ratio. You might point out to the Federal authorities that by picking the two end points of the line, they have exaggerated the variances as much as possible, because the variance of the predicted value of y is of course a function of the square of the distance from the x bar to the value.

DR. NEWILL—As far as I am aware we have not considered any planned experiments on a national scale. There are several reasons for this. One is that resources are scarce, and an effort to do this kind of thing would be extremely expensive. The second is that we would be using the general public as a testing system, to which there is a great deal of aversion. In human experimentation in this country, one must have informed consent for participation. This would make it rather difficult, since whether you think you are only looking at the different strategies or not, you are in fact involving the population.

DR. HROMI—I like the question that was raised from the standpoint that any experimental program, no matter how complex, requires some planning and forethought. On a number of occasions Dr. Finklea mentioned the desirability of accumulating certain kinds of data. This is basic to other research problems, too. We need to think through an experiment before we conduct one. We should decide what kind of data to collect and how much data to gather and what to do with them *a priori* to actual experimentation.

DR. JOHN GOLDSMITH—The experimental data to which the previous questioner addressed himself are accessible in the sense that epidemiologists have a chance to observe results of natural or technological perturbations. That applies, for example, to the requirement for certain types of motor vehicle

exhaust control in California, at an earlier date than in the rest of the country. It is conceivable, that additional data sets can be analyzed especially with the kind of approach which Prof. Box earlier presented to this Symposium. One could detect the contribution of various types of control systems in several types of communities, but the control of all other measured variables may by no means be sufficient. There is an increase in the proportion of motor vehicles which use fuels of different class, in relation to the requirements of the engine as determined by the manufacturer. For example, the sulfur content of fuel sold in the southern California basin is higher than in other parts of the country. Climatic as well as meterologic conditions may affect the resulting pollution levels. Nevertheless, we do know a lot about how some of these variables behave.

I might add that the impending use of catalytic exhaust control systems in California provides a new and perhaps even more important opportunity for obtaining such data.

Dr. Hromi, it seems to me that the approach and the questions you raised about the deterioration factor are a matter of some consequence. Of course there are mathematical relationships between the variance of a ratio and the variance of its numerator and denominator. However, one must have all of the data relevant to the numerator and denominator, and as one who has only occasionally looked at emission data, I find some peculiar truncations. Unless all of the data obtained in such a series are available, the estimates of variances that have been made, at least in the open literature, are rather peculiar. The truncations that I have observed show a clustering of values just below the accepted emission standards set by the California Air Resources Board. Would you like to comment?

DR. HROMI—Your comment on truncation is an interesting one. It leads me to mention truncation in another sense:

When a deterioration factor is calculated according to the Code of Federal Regulations and turns out to be less than one, we then must assign to it a value of one. A truncated deterioration factor distribution results.

My involvement with this problem is rather recent and my background is unlike that which you appear to have, yet I think it is easy to understand that we need to know more about the quantities that we are asked to analyze and interpret.

DR. HAROLD PECK (Merck)—Speaking as a consumer, it is desirable to have those ideal conditions where you have no emissions whatever as opposed to the situation in which there is no control. Obviously, we are going to have something in between complete control and no control. These controls cost money in the terms of original cost, in terms of fuel economy, in terms of maintenance and probably replacement. When is the public going to rebel because of excessive cost? There is already a lot of concern about the difficulty of starting cars, keeping them running until they get warmed up, and the increased use of fuel, particularly in the fuel shortage. How can we calculate the point at which the public will break?

DR. NEWILL—I don't know. Certainly public opinion is an extremely important thing in determining research priorities both in terms of what is done in the agency and the constraints that are going to be placed on regulation. There are many social variables that are just not being looked at in terms of any of these things. I can only say that this is another one of the areas we should be taking into consideration. In many ways the energy crisis has been very helpful because people have begun to discover what part of their transportation is essential and what isn't. I think both the public and the people within some of the agencies are better able to look at these problems than they were a year ago.

DR. KIRCHOFF—I have a few comments. First of all, concerning Dr. Peck's question about public tolerance and just how much the public can stand, it doesn't appear to be in the Clean Air Act at all that EPA is to worry about the economics of environmental control—it is simply to get the air as clean as possible. If the public really demands a different approach, then it will probably have to go through the Federal legislature. Dr. Hromi, I actually wanted to talk to you a little bit concerning your presentation. I have a couple of questions and, before you respond, a statement about statistics. The questions are: How many automobiles of a given class were used in the durability tests? How many automobiles were used as the certification vehicles? The comment is about statistics because the argument you raised this morning was clearly statistical in nature. You argued that one way to look at a certain class of numbers is better than another way. Eventually arguments such as these will be presented to legislators or policy makers and may very well exasperate them. If I were a member of a Senate Subcommittee, I might say "Well heck, you are from the automobile company and the reason you choose one approach over the other is because you can get by with a higher level in your certification vehicles." As a policy maker, I would be hard pressed to make judgments based on statistics. Now concerning the argument you presented, you were in reality making some assumptions about the nature of deterioration—that deterioration is something that occurs in an absolute rather than in a proportional sense. It was thus not really a statistical argument at all, but rather an argument about the nature of deterioration. You could perhaps determine the nature of deterioration if you took several thousand cars out and ran them around the track for 50,000 miles, but without such an experiment, your argument is only conjecture.

DR. HROMI—I think your question is a very good one. They are questions that certainly are of untold concern to both industry and the regulators. We in industry do not choose an approach. The approach that I defined and questioned is in the Code of Federal Regulations, so we have no choice in the approach. The Code of Federal Regulations, Title 40, Part 85, contains a procedure that specifies the selection of cars by the EPA based on sales volume. In certain makes and models in which sales volume is not appreciable there is a minimal number. I don't know what our recent numbers of test cars are, but there could be as few as one per engine family—at the discretion of the EPA.

DR. KIRCHOFF—What is the maximum number that could be tested?

DR. HROMI—I have no idea what the maximum number could be, but it certainly is not a thousand.

DR. HENDERSON (Olin)—Mention was made of human experimentation, and Dr. Finklea indicated some questions about the catalytic converters that will be mandatory in 1975. It would appear to me that we are making the total US population an experimental group for a system that is possibly questionable. I would like some comment on that.

UNIDENT.—California is the only place they are going to be used in 1975.

UNIDENT.—So California then becomes the test population? Has everybody in California given their informed consent for that experiment?

UNIDENT.—In California catalysts are being introduced on a very limited scale. How much of the car population is actually changed over in a course of one year and how much adverse health effect will come from that limited edition exposed to the population? By actually introducing it in such a limited fashion you will have the opportunity

to monitor and discover whether or not our speculations are valid. The actual quantity of sulphur in gasoline that will be converted to sulphate is not very large. The problem lies in the fact that it will be concentrated close to major freeways, so that there may be much higher exposure levels than we really want. In estimating those, we have used only dispersion models. Any of you who have been part of that process know about the tremendous fight over whether the models we have used are proper or not. We can detect many of the problems only by making some kind of limited introduction into the population and observing for the suspected risks.

DR. HROMI—You indicated that some results take a long time to acquire. Dr. Singpurwalla, would you care to address that question from the standpoint of a reliability engineer and statistician who is studying accelerated data collection?

DR. SINGPURWALLA—Yes, I would. The problem that I have been working on is the analysis of accelerated life tests—that is, data collected under accelerated environments. I suspect that this is a very nice analogy wherein similar techniques could be used in environmental studies, whether they involve human populations or automobile populations. There are methods by which such failures can be analyzed based on physical hypotheses or biological hypotheses about certain failure mechanisms and those techniques could essentially be transferred to whatever extent they are feasible into the problem of this particular nature. Along those lines, Dr. Hromi, your first view graph contained a straight line for hydrocarbons on the vertical axis and mileage on the horizontal axis. Without running a regression I notice that rather than being a straight line, might it be two segmented lines?

DR. HROMI—Your point is well taken and it's one that we ourselves question. Yet I want to stress that at the moment this is the direction that was provided by the Code of Federal Regulations, and this is the way we must respond. We must draw least squares best fit lines over those points. Whether the points suggest something else is another matter.

DR. DOMEY (U. Texas)—This is far too peaceful a conference. Dr. Hromi, let me ask you a question. In 1957 the city of Seattle was the scene of a conference that had to do with the problem of atmospheric contamination by vehicles. At that time we were assured by representatives of the industry that they were hard at work on this problem. In the meantime the DuPont Corporation had outfitted several vehicles with the complex equipment capable of continuous monitoring of atmospheric contaminants. Registers in four colors for example, on a continuously-rotating paper were presented. First of all, we are here because of a mutual concern over this problem. I wish to defend my friends in Government, though I do not consider them necessarily my close friends. At least they have generated a target at which industry must direct its criticism. It matters not whether this line slopes this much or that much at the moment. That is a trivial point. My question is to industry, which has had some 15 years to collect the data. Where are the basic data?

DR. HROMI—I wish I were able to answer that question. I think I have indicated that I am not involved in emission work on a continuing basis. My occasional involvement is that of a statistical consultant. I have no idea what kinds of records were kept over the last 15 years. Dr. Domey, I would like very much, though, to take your question back. So, after the session let's formulate the question you would like to have answered and I'll try to get an answer. I'll see you after the session.

DR. BOX (U. Wis.)—I also felt very sympathetic with the last speaker. The thing that I wonder about is the provision for feedback. The Ford Motor Co. has a great deal of money and a lot of expert statisticians and they presumably have recommendations as to how this analysis ought to be made. You did hint that there is some kind of conversation going on. Is there some mechanism whereby there can be some negotiation and things can be changed?

DR. HROMI—Yes. However, let me go back a moment and say that it certainly was not my intent to make anybody else look bad. I try to focus on some problems that exist because of the state that we are in at the moment. When I started to write the paper I was privy to some information that existed in the '72 and '73 version of the Code of Federal Regulations. I was reminded recently that there have been many changes in those regulations that have occurred over the last three or four years as a result of dialogue, some formal and some informal, with people in the administration. As far as I know, there isn't any routine mechanism for providing feedback.

DR. BOX—There's nothing like a hearing where people can just come and give evidence?

DR. HROMI—I know of none.

DR. BOX—Just one other comment on the question of ethics—of the public being experimented on and so on. It seems to me that we are always in this situation to some extent. In the testing of cancer drugs and so forth—first, there are benefits; second, there are possible dangers; and third, have we done everything we can do before we get to this point? It is clear there is a great deal of information already potentially available possibly which is viable but not yet fully exploited. For example, there is a 17-year record of hourly measurements of several pollutants at a number of different locations in Los Angeles County. We were asked at Wisconsin about 18 months ago to (a) try to get the data out so that it was available for everyone, and (b) try to discover its significance. This meant that we had to invent some statistical methods. We've been working pretty hard on that. I imagine there are other sets of data as well. The fact is, in Los Angeles they have introduced a number of new regulations from time to time. I suspect that some of these regulations have made no difference whatsoever. As far as I know they have not repealed those laws. But there are some effective laws, and the effects have not been exactly what one would have expected. Others of us can learn from this experience.

The American public is somewhat spoiled and I think it may be a very good thing if it is in for a period of deprivation. It may set some values straight. Americans live in a poor world and are going to go the way of other aristocrats unless something is done about it. I could manage with less money than I do. I don't really need two cars. I would be a lot better off if I cycled to work. Having an energy crisis, and reorienting our priorities, and the fact we are not going to be fooled into buying big cars we don't need and very often using drugs we don't need either is going to be a very good thing. And I remember that in England during WW II when we had fair rationing, we got 10-penny worth of meat but we knew that nobody else was getting more, and we had a pretty good time. People had a purpose then, and the war years held some of the happiest times I remember. Shortages don't necessarily mean that things are going to be black—they are perhaps just going to be more interesting.

DR. HROMI—Something else occurred to me this morning after George made a point about having a formal channel for a continuing dialogue. Fred Leone, one of the organizers of this Conference, talked with me. Fred said that one of the purposes of this Con-

ference is to try, at least on an informal basis, to establish more healthy dialogue between the kinds of people in this room. But then he asked the question, "What next?" I think that bears on what you have in mind, George. A result of this symposium should be a continuing kind of forum such that, even though my remarks might have sounded like they were intended to be maliciously critical, can be regarded as constructively critical. We need a forum where we can put these problems out on the table.—where we can attempt to solve some of these problems with the kind of knowledge we already have, or define needs for new knowledge, as you indicated was necessary in the California study.

DR. MARCUS (U. Md.)—Dr. Kirchhoff, you talked about the possible extremely serious potential health effects of pollutants from the internal combustion engine. We have talked about one control strategy, which consists of putting some sort of Rube Goldberg device on the tailpipe to be obscurely evaluated, argued about, and probably disconnected in appreciable numbers. There are other alternative transportation control strategies. The core of the problem is that about 40% of the workday motor vehicle trips in urban areas are made during the four peak hours with an average ridership of about 1.3 people per car. It seems to me it should be the purpose of the Department of Transportation as much as EPA, to do something about the control strategies in that area. Are the air pollution emission consequences of some of the strategies being monitored or assessed and to what extent? Are they being developed?

DR. KIRCHOFF—That is a double barreled question, part of which I cannot answer. As you know, car pooling is one measure to reduce peak transportation demand. Mass transit and the incentives which have been placed on the expansion of mass transit are measures which the Department of Transportation has taken seriously, not so much to reduce pollution but to conserve energy. I think Dr. Newill can probably tell you more about monitoring of emissions. Traffic volume will surely be monitored very carefully and various areas will be combining these data with emission data to find out exactly what the effects are.

DR. NEWILL—The implementation plans that are required for an area to actually meet the ambient air quality standards requires monitoring. A great deal of monitoring is being done, much more than a few years ago, so that there will be data available. The intimation has been that the energy crisis hasn't gone for a long enough period of time for us to actually have those data. There are several reasons for that. One reason is that the data probably still reside in the places where they're originally gathered, not yet having reached the offices where they will be evaluated. I'm sure that a great deal of monitoring is going on for most of the pollutants we are discussing here.

The EPA has recognized that there hasn't been as good a forum for the scientific community as there should be. Industry, more than the scientific community in general, has availed itself of a direct opportunity by asking for meetings and presenting their point of view. They haven't always had a time reaching the people within the agency as they probably should have, and this was part of the impetus behind the establishment of a Science Advisory Board within the Agency, the approval for which was published in the Federal Register on January 18. The Board is being assembled at the present time. I think this will be a mechanism whereby the scientists can, in fact, communicate with the agency in a fashion differently than they did before.

I have talked to Fred Leone about the possibility that this association should ask the Agency to place some statistical talent on that particular Board—

someone who can reflect to the administration the need for this kind of thing. We are talking about all the short-term solutions, but they will take a very long time to come—generations. One of the biggest things we have to change are people. All of the problems we are talking about here should be taken particularly seriously by those in academic communities. Here is where they can think unencumbered about these long-term problems and can begin to train a new generation of people to handle them in a different fashion than in the past. As with so many other things, time will put the problem into perspective. These short-term effects are certainly not going to be disastrous.

DR. HROMI—You made a comment that reminded me about the mechanisms for establishing a dialogue between the regulator and the regulated. I don't know what your agency's mechanism is. I am somewhat more familiar with the opportunity to discuss proposed rules and regulations before they become law in the National Highway Traffic Safety Administration in DOT. Notices of safety standards are posted in the Federal Register and the whole world has an opportunity to respond before the proposal becomes a law.

DR. KIRCHOFF—There is always a problem in communication between regulator and regulatee in that the relationship is primarily an adversary one. Perhaps this is because the government is run by lawyers. I don't think scientists are comfortable with such an adversary relationship. We would rather approach problems from the standpoint of ascertaining the truth of a particular idea rather than to present arguments supporting a particular point of view and to subject these arguments to judgment. Unfortunately, this adversary relationship persists in the public hearings related to environmental legislation.

UNIDENT.—I think it is time to express a little bit of the 30-year experience of the AEC with some of these problems. Relative to the public having a chance to have its opinion brought to bear, the Agency does respond to the legislative and executive branches of the government—it is really through them that the public has its first avenue of expression. Public hearings have become standard, and adversary relationships are going on constantly. However, getting into the scientific arena does not eliminate the adversary nature one bit. In fact, the situation is made even more difficult. Our most trying adversaries in the atomic energy business are some of the world's best scientists. This is an intellectual challenge that makes it good fun. However, the suggestion that the informed public should have a plebiscite on the types of regulations that are made leads to "every man for himself" in the case of unrestrained advocacy. In this regard I think that the AEC and EPA are sometimes in confrontation. In their defense, we have to respond to public needs and interest within the limits of enabling legislation, with full opportunity for the public to have redress through the judicial process. It is a long, tedious procedure but it is available and it certainly is better than the plebiscite approach to the problem.

We have been challenged lately on the possibility that radiation standards are not set for susceptible groups of the population. This is true—radiation standards essentially define the standard man. The evidence is considerably stronger now that there are susceptible sub-groups with regard to air pollution problems. Dr. Newill, how do you rationalize the problem of recognized susceptible subgroups in regard to setting risks?

DR. NEWILL—We are having a great deal of difficulty with this problem. At the present time we interpret the Clean Air Act to mean that you want to protect the people who are particularly susceptible to respiratory problems against an increase in the number of symptoms they have. In the

air pollution area I think we are doing a very good job. Other areas are more difficult. It will be some time before we have the same philosophical framework for our regulations and every media and categorical program. One of the reasons for this is that we operate under different legislative mandates in these different areas. A good example was that Mr. Ruckelshaus' statement about the reduction in the amount of traffic to meet the clean air standards in the Los Angeles area were really borne out of frustration that he had no flexibility in the way he could achieve them except to delay things for a year or so. He had to bring home to the people what the consequences were, and you will have to admit it did start a dialogue.

UNIDENT.—I am a statistician with the EPA. One must look at the purpose of the deterioration factor rather than worry about the specifics of it. The idea is to control the total emission over time of vehicles to set some sort of method of estimating what total emission true time will be. It is immaterial whether a least square fit is appropriate, or whether a normal distribution or a segmented function might be better. Rather, is this the best way of establishing a method for controlling total emission? That is the only criterion that should be used for judging these functions.

DR. KENNETH BUSCH (NIOSH)—Whether a ratio or a difference is appropriate between the 4,000-mile and the 50,000-mile values is one matter which can be determined by testting real vehicles and taking more data—it is an engineering problem that should be solved. The reason for using more multiple points on a fitted model is to smooth out the data and gain the advantage of precision from the additional monitoring data. You are predicting, from results of the test vehicle at 4,000 miles, a value which it would attain at 50,000 miles. Assuming that the difference model is appropriate, the prediction would have a confidence interval centered around the predicted value, so that 50% of the time the true value would be above and 50% of the time below the prediction. This means we are running a 50-50 risk of being higher or lower than the intended standards. Should we not use the upper confidence limit as a point of comparison, rather than predicted value?

DR. SINGPURWALLA—We have been confusing technical and nontechnical issues. One individual says that it's not important whether it is a straight line or a normal distribution or a segment. Someone else agrees with him but says it should be looked at as a confidence level limit—we are getting into technical issues. I don't know what the answer to that is, but if we have the technical skills and the abilities to look at a problem as precisely as we can, why should we not look at it that way? Therefore, I challenge the statement that it is something gross that we should look at. . . .

UNIDENT.—I don't think it's gross. Let's answer the real question—not to get the best fit we can but to do the best job of answering the real question. These are two different things in this case.

DR. SINGPURWALLA—But would we be able to answer it better if we look at it properly?

UNIDENT.—Properly, yes, but properly may not be a least square sample.

DR. SINGPURWALLA—Oh, I agree. But as Dr. Schneiderman said yesterday just be sure you're asking the right question when you answer it better.

DR. JAMES TAYLOR—As an economist who is interested in fuel and energy, I would like to raise the point of gasses from high stacks, in electrical power plants for example. This is a problem that involves billions of dollars

of expense currently to the American public in capital investment and in expenditures for low sulphur fuels. Take the case of a large coal-burning electric power plant in southern Nevada, completed in 1971. To conform to the country air-pollution regulations, they say they must spend $100 million, provided an economical method can be devised for removing sulphur and other noxious gases from the emissions. The concept of the ambient air stream being affected by this kind of situation has been publicly challenged by Dr. Philip Abelson, President of the Carnegie Institution, in his annual report last year. He points out that more sulphur is put into the ambient air stream every day by nature than by man. It seems almost impossible to achieve the President's goal of self-sufficiency in fuel and energy production in the United States in the next 5–15 years unless large particles of coal with a good deal of sulfur content are burned. If we are to avoid that, we must spend a perfectly prodigious amount. I have heard nothing about this major problem in air pollution control, or anything about water control.

DR. NEWILL—One of the things that worries me is what the use of coal will to do to the environment *per se*. It is true that taking sulphur out of coal costs money, but many of the new processes such as coal gasification and liquefication will result in a much cleaner fuel. I wish that the wisdom to invest more money in those processes had resulted in some earlier budgets that would have allowed us to have the technologicals now. However, we weren't that wise, and we probably will suffer from some increase in adverse health effects. That nature puts more sulphur into the atmosphere than does industry doesn't concern me in the least. What does concern me is its effect on people. Industrial sulfur in the atmosphere is a distributional problem. We must determine how much risk people are willing to tolerate from their exposure. This has nothing to do with the energy crisis except that it might increase their tolerance a little bit. It doesn't mean that we should give up the idea of protecting people from these pollutants. Certainly we have to have short-range solutions to the problem, but the long-range goals should not be changed by the energy crisis. I don't worry very much about electrical power plants because the total amount of money being spent is nothing tremendous.

DR. HOMAN (Nat. Cancer Inst.)—I am a toxicologist. Some concern has been expressed by handling deterioration factors of less than one. A valid deterioration factor of less than one tends to imply that pollution controls, like good wine, improve with age. If you reject this contention, what is suggested with respect to data that produced such a number?

DR. GOLDSMITH—I would like to introduce a fairly important statistical problem—that of available data sets on air quality—which I think would yield useful results with some additional attention. I refer to the requirement under the regulations of EPA for establishing emergency plans. In California we are expected to notify people so they can take protective measures when we think something unusual may occur, such as high levels of air pollution over a specific period of time or at a specific location. Very often, available monitoring station data cover past periods. We currently face two classes of problems of a statistical nature for which I think some practical statistical applications would help us a great deal. The first problem pertains to a systematic use of sampling strategy to determine how well monitoring is located to measure what people are breathing. We have no reason to assume *a priori* that a given monitoring station is sensing the same air that is breathed by a given population, yet we have every reason to determine how these two are related. Collectively I think we who are espe-

cially concerned with health have been somewhat negligent because if we had been a little more articulate, perhaps it wouldn't have taken so long to get our point across to those who operate the monitoring programs. At present we don't know what monitoring stations represent in terms of area or population exposure. The second problem has to do with monitoring system data now available which will help us predict within certain probability limits how much exposure will exist at some time in the near future. For example, can we predict at 6 AM that it would be better to carpool than to drive one's individual car? While there is a good deal of discussion about car pooling, nothing is being done to facilitate it. There is no arrangement for providing gasoline; the very real economic incentives are very poorly documented; and there are usually no facilities for gathering people who want to ride in the same direction, although there are boards in which people are supposed to put cards. It's very difficult for car poolers to foregather in some windswept corner. Therefore, I ask the panel to suggest some practical way to solve these two statistical problems.

UNIDENT.—Regarding the car pooling situation, it is true that you see boards, but if you don't have a radio, you don't hear station WTOP promoting the car pool. Various industries are computerizing car pools, and a number of communities and industries throughout the country are using these computerized programs for car pool matching with various incentives. I believe Minnesota Mining has bought a number of 12-passenger minibuses for employee car pools. We would like to see ridership increase here in Washington from its present 1.6 to about double that. We feel that this would drop our peak hour concentrations 20% or better. Car and bus pooling has been very much in our minds and we are doing what we can about it, but it's awfully hard to wean the American from his personal transportation. I've been told that if gasoline goes to a dollar a gallon we'll have no problem except malnutrition, because some people will pay a dollar a gallon for gasoline and eat fried potatoes from then on and not get out of their car.

DR. KIRCHOFF—I wanted to make a comment concerning Dr. Finklea's mention of the EPA's problems with the NO_2 measurement techniques. Because of the unreliability of the EPA Reference Method for the determination of NO_2 a great deal of important data may have been irrevocably lost. A unique situation existed in Chattanooga in that a TNT plant was a prominent source of NO_2 in a rather local area. Health studies were made of people who were exposed to the NO_2 and people who weren't. These health studies, which relied on the NO_2 measurements for the determination of exposure levels, were critical in the setting of national primary standards and automotive emissions standards for NO_2. Well, the war in Vietnam is over and the TNT plant has closed and repetition of the study is no longer possible. A detailed description of the effect of the discovery of the unreliability of the NO_2 measurement method on the National Air Quality Standards and on the automotive emission standards appears in the June 8, 1973, Federal Register. A large amount of data is presented and I invite the statisticians in the audience here today to take a look at it. If anything, it should convince you of the need for a sound statistical and scientific basis for environmental decision. Information such as this is in the public domain whether published in the Federal Register or available from EPA under the Freedom of Information Act. Go look at it and work with it!

DR. LEONE—You just hit a sensitive nerve when you said that the information is there—go look at it. I don't think that is really what we want. Rather, let's get information together, talk about it together, plan the way we get it, and go ahead. We are trying to

agree to talk before the decisions are made—together we will talk about the risks, about how we get the data, about whether the data is meaningful, and about potential conclusions relative to the type of data we get. Not communicating is the thing we have to overcome.

DR. KASTENBAUM—The following quotations are from the book, "Geography", by Henrik William van Loon: "We are, all of us, fellow passengers on the same planet. We're, all of us, equally responsible for the happiness and well-being of the world in which we happen to live." "We have plundered it all in less than a century without paying any attention to the interests of those coming after us." Both these quotations relate to some of the statements made by George Box yesterday.

As a result of much of what has been said today, I have the feeling that many of us are acting as if we have just invented the wheel. We have only to examine the vast literature on the effects of ionizing radiation to realize how naive and inaccurate such an attitude is. Indeed in the area of radiation biometry, many concepts of interest to statisticians and environmentalists, such as doses, dose-rates and thresholds have been considered and discussed at considerable length. A National Academy of Science report released just a few weeks ago devotes an entire section to the concept of low dose. The amazing thing about this is that the committee responsible for writing the report found it necessary to devote a section to a discussion of this apparently simple concept, in spite of a fifty year history of research and literature on an agent which is known to be carcinogenic, mutagenic, and teratogenic. This report is entitled "Research Needs for Estimating the Biological Hazards of Low Doses of Ionizing Radiation". I recommend it to all serious students of the application of statistics to problems of the environment. Two other comprehensive studies of the effects of ionizing radiation are at least as important. These are:

1. BEIR; *The Effects on Populations of Exposure to Low Levels of Radiation,* National Research Council (1972).
2. UNSCEAR, A/8725: G.A. Official Records, 27th Sess. Suppl. No. 25 (1972).

MR. WANDS—During the course of our discussion yesterday and this morning, three words were bandied about—"risk," "benefit," and "analysis." So far we have focused our attention almost entirely on risk measurement and analysis but have touched very lightly, if at all, on the question of benefits. An administrator must resolve this very important side of the equation in setting some kind of regulatory standards. I grant that the data are even fewer and more unmanageable in the area of benefits than they are in the area of risk, but it is time for us to begin planning a concentrated approach to quantifying benefits. It's the old question of equating dollars with lives or marginal illness, etc., but there is still much to be done before we can achieve the long-term rational approach to the goals of which Dr. Newill has just spoken.

In response to the last speaker, one of the reasons this Symposium is being held at this particular time is because of the Environmental Mutagens Society meeting this weekend and, following that, the Society of Toxicology. This does assure a potential at least of half the interested scientific communities being in town and available, particularly since we wanted to make this Symposium nationwide rather than local as the two preceding ones were. We were very hopeful that particularly the radiation biometry group would be in our audience to share their experience with us, even though we are focusing our attention today on the problems of chemicals entering the environment.

Yesterday and again today we heard statisticians Nancy Mann and Dr. Singpurwalla mention the use of inten-

sive testing for failure as a means of predicting ultimate long life. This is fairly straightforward in terms of mechanisms that are simple and well understood, such as flex fatigue in metal strips or of paint failure under sunlight, radiation, etc. However, in biological systems one usually finds two entirely different mechanisms—one in relationship to the short-term, heavy-dose exposure and the other to the long-range, low-level exposure. Standard techniques within the field of toxicology are available for doing intensive short-term studies. Sometimes it is as short as a single dose, for example, determining an LD_{50}. More intensive, repeated doses once were used by the National Cancer Institute in its chemotherapy screening program in which animals were dosed at least twice a day for seven days at a maximum tolerated dose. We wanted the animals to stay at least barely alive so that we could study the effects of the chemotherapeutant on the animal carrying the experimental tumor. Perhaps Dr. Schneiderman would like to comment on the statistics that were used in evaluating those experiments. There is also a thirty-day feeding study which lasts a little bit longer than a single dose or a daily dosing. Sometimes this is modified by increasing the dose every week to the point of failure of the test system; ie., death of the animals. Perhaps Caroll Weil, who is in the audience and is quite familiar with the statistics commonly used in the field of toxicology might like to rise to that issue.

Last night's Washington-Star News [Mar. 6, 1974] carried in the women's section a big front color spread on the nitrite question. Attention, of course, is being focused on nitrites in our food. Two or three times during our discussions yesterday and today we have had some rather vague, but nevertheless real, suggestions that the oxides of nitrogen which are inhaled might ultimately react with some of the body proteins or amino acids to form these nitroso amines which are of concern in our diet, particularly those meat products which are preserved with nitrite. Congress has established a system for protecting the public health based upon routes by which toxicants enter our bodies. For example, FDA controls what we eat, EPA administers one law controlling the air we breathe and another controlling our drinking water. The problem is that there are many substances, such as the nitrosamines, which enter our bodies by several of these routes. Dr. Finklea gave us the example of lead in his paper this morning. Yet, there is no concerted effort to correlate the controls of these multi-entry insults to our bodies.

I would like the panel, particularly the statistician, to discuss how to tackle the nitrite problem. We know that the nitrosamines are formed in some foods containing nitrite, for which there is at present no substitute. We have been eating such foods for over a century and during that time some people have developed cancers.

DR. SINGPURWALLA—I appreciate the complexity and the magnitude of the problem, but it is not something that I can answer in a minute.

DR. HROMI—I think I can paraphrase what you said. One needs to understand what the long-range problem is before he can respond to it. That does appear to be a rather complex problem, and to try to respond on the spot is difficult.

DR. BOX—I would like to return to a question raised some time ago in the discussion by Dr. Goldsmith concerning the relation between measured levels of pollutants and levels actually breathed. One thing that is clear is that the level measured may be far less reliable than people imagine. In the records that we have been analyzing, for example, dramatic changes in apparent pollution levels can be traced to changes in location of instruments and to changes in carrying out the details of the analysis. Because reproducibility at

a given station is high, one can easily be lulled into believing that a measurement is accurate. Cooperative studies are needed on a continuing basis to provide checks.

DR. ROTKIN—I am here as an individual, so this comment is an expression of purely personal prejudice. Dr. Hromi, I was happy to hear a paper that, instead of saying what factors should be considered, actually considered them.

I don't think you should deplore adversary relations. Unless you consider them, this Symposium has an unreal air about it. Whenever you deal with problems on which people will either have to devote energy or spend money, it is unrealistic to seek the best solution from an overall humanitarian point of view. If someone must extend effort or some fortune to achieve this result, you can expect him to put up a fight to oppose it. And there is no use devising a nice procedure for helping humanity if you ignore the fact that you will get opposition—you might as well consider who will oppose you, and why, and what you can do about it. This implies adversary relations.

Concerning the indignant remark about one of the early comments regarding how long people will put up with this—will people have lost their patience? One of the first clean water acts was passed during the 19th century. People got sick and tired of their water being made dirty by all kinds of pollutants. They lost their patience again more recently when they began to find soapy foam in every stream and when several people near highways died of suffocation because of inversion. People lost their patience a long time ago—you don't have to ask when they will lose it.

It's odd that when the government or some academic group wants to conduct experiments involving humans, people worry about the ethics. Nobody considers the ethics when a manufacturer introduces a new hair spray that millions of women will breathe. Nobody worries about these guinea pigs. Nor when someone introduces a new soft drink the label on which lists water as the only natural ingredient—everything else is one chemical or another. It is made to taste like raspberry juice, but there is no shred of raspberry in it. I'm sure you can think of many other examples.

Now, my specific objection to your paper, Dr. Hromi. You objected to a ratio—you said that an arithmetic difference might be a better way to look at it. I suggest that, especially when you deal with catalysts, you should consider the possibility that deterioration will increase as the level increases. Perhaps you should have, rather than a ratio, some kind of an exponential which would make matters worse for the company rather than better.

DR. HROMI—If this question-answer period is typical, perhaps our Symposium is achieving its purpose. Being adversaries in a friendly kind of atmosphere like this is helpful. Thank you.